THE JOURNAL OF PHILOSOPHY

VOL. LIV, NO. 10: MAY 9, 1957

CONTENTS

Wittgenstein on Language and Some Problems of Philosophy: LEONARD LINSKY 285

Professor Goodman's "Fact, Fiction, & Forecast": JOHN C. COOLEY 293

New Books 311

PUBLISHED FORTNIGHTLY

BY THE

JOURNAL OF PHILOSOPHY, INC.

THE JOURNAL OF PHILOSOPHY

FOUNDED BY FREDERICK J. E. WOODBRIDGE AND WENDELL T. BUSH

EDITORS: Herbert W. Schneider, John H. Randall, Jr.

CONSULTING EDITORS: Ernest Nagel, Alexandre Koyré, José Ferrater Mora.

MANAGING EDITOR: Emerson Buchanan.

Communications to the editors should be addressed to the Managing Editor, 713 Philosophy Hall, Columbia University, New York 27, N. Y. Subscriptions and advertisements should be sent to THE JOURNAL OF PHILOSOPHY, 713 Philosophy Hall, Columbia University, New York 27, N. Y. The annual subscription price (twenty-six numbers) is $5.25. The price of single numbers is 35 cents, double numbers, 60 cents.

THE JOURNAL OF PHILOSOPHY is owned by the Journal of Philosophy, Inc., and published fortnightly at Prince and Lemon Sts., Lancaster, Pa. Editorial Office: 713 Philosophy Hall, Columbia University, New York 27, N. Y. President, Albert G. Redpath; Vice President, John H. Randall, Jr.; Treasurer, Frederick A. Dewey; Secretary, Ernest Nagel.

Entered as second class matter, March 18, 1921, at the Post Office at Lancaster, Pa., under Act of Congress of March 3, 1879.

Acceptance for mailing at special rate of postage provided for in paragraph (d-2), section 34.40, P. L. & R. of 1948, authorized March 11, 1952.

PUBLICATIONS

FOR SALE AT

THE JOURNAL OF PHILOSOPHY INC.

515 W. 116th STREET NEW YORK 27, N. Y.

A Bibliography of Philosophy, 1933, 1934, 1935, 1936. 4 issues, 56 pages each. (Double numbers of THE JOURNAL OF PHILOSOPHY.) $1.00 each issue, $2.75 for the four issues.

Cosmogony. By Christian Ehrenfels. Translated from the German by Mildred Focht. xii, 223 pp. $2.50 (cloth cover).

Philosophy and Method. By Richard McKeon. Reprinted from THE JOURNAL OF PHILOSOPHY, Vol. XLVIII, No. 22, October 25, 1951, pp. 653–682. 35 cents.

Negation. By A. J. Ayer. Offprint, from THE JOURNAL OF PHILOSOPHY, Vol. XLIX, No. 26, December 18, 1952, pp. 797–815. 15 cents.

VOLUME LIV, No. 10 MAY 9, 1957

THE JOURNAL OF PHILOSOPHY

WITTGENSTEIN ON LANGUAGE AND SOME PROBLEMS OF PHILOSOPHY

IN the *Philosophical Investigations* Wittgenstein asks, "Is what we call 'obeying a rule' something that it would be possible for only *one* man to do, and to do only *once* in his life?" (199).[1] The question, Wittgenstein observes, is a grammatical one calling not for empirical investigation but a logical analysis of the concept of obeying a rule. Wittgenstein says that it is not possible that there should have been only one occasion on which someone obeyed a rule. Of course, we can imagine all sorts of situations in which a rule is promulgated, obeyed only once, and then withdrawn. This is not what Wittgenstein is talking about here. He is talking about the practice of obeying rules, not this or that particular rule. In the same way we may distinguish between the practice of playing chess and this particular game which falls under that practice. Of course, a case cannot fall under a practice unless the practice already exists. It is not possible that there should have been only one occasion on which a report was made, or an order given and understood. It is not possible that only once in the history of man there was such a thing as making a report. For making a report is conforming to a certain practice or use. There cannot be a practice without cases falling under the practice any more than there can be cases falling under a practice without a practice.

Suppose we are asked why we call a certain activity "giving an order." Perhaps we are asked this question because certain features of this activity were unnoticed. We might then point them out in explaining our use of words. If this process is carried out long enough we may finally reach the point at which we say, "But this is the way our language is used." And in doing so we appeal to the customs, and uses of our language which make the expression "to give an order" apply in just this case. But now imagine the background of custom removed, and you will see that

[1] All quotations are from *Philosophical Investigations* by Lugwig Wittgenstein (Oxford, Basil Blackwell, 1953). The numbers between brackets refer to numbered remarks of Part I of this work.

the possibility of distinguishing between a correct and an incorrect use of words has disappeared as well.

Now try to imagine a private language. Here the background of customs is also private. What would a private custom (practice) be? What would a private rule be? Well, I might call the object before me a "took" instead of a "book." And in a sense I would be speaking a private language. But Wittgenstein is not talking about this sort of thing when he says that there cannot be a private language. "Following a rule is analogous to obeying an order. We are trained to do so; we react to an order in a particular way. But what if one person reacts in one way and another in another to the order and the training? Which is right?" (206). In order to answer this question we must refer to the common behavior of the community of men who are using this language. The right way of reacting to an order must be the customary way. Without the rules embedded in these customs, there is no system of reference by which we can justify the claim that one response is the right one and another response the wrong one to a particular order.

When Wittgenstein discusses private languages, he has in mind languages which could not possibly be understood by anyone other than the speaker. Malcolm has observed [2] that the idea of just such a private language is presupposed in a host of philosophical programs and theories. It is involved in the problem of our knowledge of other minds. That problem arises when we suppose that in the case of our sensations, our language is a private language. It is only from my own case that I know what is meant by pain. I experience only my own pains. But if the meaning of "pain" is known to me only from my own case, from my own private exemplar, it becomes a mystery as to how anybody else can understand me when I use the language of pains. Only I can experience my pains. Similarly, when others speak of pains they are referring to their own exemplars which are inaccessible to me. For all I can really know, they may be referring to nothing at all!

Let us imagine the following case. I want to keep a diary about the recurrence of a certain sensation. To this end I associate it with the sign "E" and write this sign in a calendar for every day on which I have the sensation.—I will remark first of all that a definition of the sign cannot be formulated.—But still I can give myself a kind of ostensive definition.—How? Can I point to the sensation? Not in the ordinary sense. But I speak, or write the sign down, and at the same time I concentrate my attention on the sensation—and so, as it were, point to it inwardly.—But what is this ceremony for? for that is all it seems to be! A definition surely serves to establish the meaning of

[2] Norman Malcolm, "Wittgenstein's *Philosophical Investigations*," *Philosophical Review*, Vol. LXIII (October, 1954), pp. 530–559.

a sign.—Well, that is done precisely by the concentration of my attention; for in this way I impress on myself the connexion between the sign and the sensation.—But "I impress it on myself" can only mean: this process brings it about that I remember the connexion *right* in the future. But in the present case I have no criterion of correctness. One would like to say: whatever is going to seem right to me is right. And that only means that here we can't talk about "right." (258)

In this remark, Wittgenstein is saying that a private language would be one in which there could be no criteria for distinguishing the correct from the incorrect use of language. Our own (public) language does provide such criteria. The criteria for the correct application of words for sensation are taught and learned in many kinds of public situations. When we try to imagine such learning situations, it becomes apparent that the private exemplar is not needed at all.

It might be said that there are indeed criteria for the correct use of my private language. All I have to do is to appeal to memory to see if I have used, for example, the sign "E" in accordance with my private intention. To this Wittgenstein says:

Let us imagine a table (something like a dictionary) that exists only in our imagination. A dictionary can be used to justify the translation of a word x into a word y. But are we also to call it a justification if such a table is to be looked up only in the imagination?—"Well, yes; then it is a subjective justification."—But justification consists in appealing to something independent.—"But surely I can appeal from one memory to another. For example, I don't know if I have remembered the time of departure of a train right and to check it I call to mind how a page of the time-table looked. Isn't it the same here?"—No; for this process has got to produce a memory which is actually *correct*. If the mental image of the time-table could not itself be *tested* for correctness, how could it confirm the correctness of the first memory? (As if someone were to buy several copies of the morning paper to assure himself that what it said was true.) (265)

The difficulty with the notion of a private language is that in it there is no difference between seeming to be correct and being correct. If it seems to me that I am using "E" in accordance with my remembered intention that is as far as I can go toward settling the matter. But if there is no difference between seeming and being a correct use, doesn't the notion of correctness itself, and with it the idea of language, become inapplicable?

Let us remember that there are certain criteria in a man's behavior for the fact that he does not understand a word, that it means nothing to him; that he can do nothing with it. And criteria for his "thinking he understands," attaching some meaning to the word, but not the right one. And, lastly, criteria for his understanding the word right. (269)

Wittgenstein's point here seems to be that the expressions "to understand a word," "to appear to understand a word," are ex-

pressions of our common language. The criteria for their correct use are public criteria. That these criteria are public and can be taught and learned, in spite of the supposed inaccessible private exemplar, is shown by the fact that all of us do in practice know how to use the expressions involved. It cannot, then, be correct to call any of these supposed private and inaccessible processes "defining" or "understanding," for these words belong to our common language and the criteria for their application are publicly taught and learned. Wittgenstein makes this point in the following remark:

> What reason have we for calling "E" the sign for a *sensation?* For "sensation" is a word of our common language, not of one intelligible to me alone. So the use of this word stands in need of a justification which everybody understands.—And it would not help either to say that it need not be a *sensation;* that when he writes "E", he has *something*—and that is all that can be said. "Has" and "something" also belong to our common language.—So in the end when one is doing philosophy one gets to the point where one would like just to emit an inarticulate sound.—But such a sound is an expression only as it occurs in a particular language game, which should now be described. (261)

There is another side to Wittgenstein's view on the nature of language which is closely connected with these topics. This is what has been called his instrumentalist theory of language. This instrumentalist view is the center of gravity of Wittgenstein's philosophy. We can approach it first from a consideration of the idea of a language game. A language game is a use of language for some purpose.

> Now think of the following use of language: I send someone shopping. I give him a slip marked "five red apples." He takes the slip to the shopkeeper, who opens the drawer marked "apples," then he looks up the word "red" in a table and finds a color sample opposite it; then he says the series of cardinal numbers—I assume he knows them by heart—up to the word "five" and for each number he takes an apple of the same color as the sample from the drawer.—It is in this and similar ways that one operates with words. (1)

And consider again this example of a language game:

> The language is meant to serve for communication between a builder A and an assistant B. A is building with building stones: there are blocks, pillars, slabs, and beams. B has to pass the stones, and that in the order in which A needs them. For this purpose they use a language consisting of the words, "block", "pillar", "slab", "beam". A calls them out;—B brings the stone which he has learnt to bring at such-and-such a call.—Conceive this as a complete primitive language. (2)

In these descriptions the view of language as a tool, an instrument, is prominent. The language game is a whole "consisting of language and the actions into which it is woven." This view of

language as a tool is in complete opposition to Wittgenstein's earlier view presented in the *Tractatus Logico-Philosophicus,* that language is to be regarded as a picture or mirror of reality. Much of the *Philosophical Investigations* is devoted to replacing the picture theory by the tool-box theory of language. "Language is an instrument. Its concepts are instruments" (569). Instruments may be classified according to their methods of operation and the functions which can be performed with them. The notion of the meaning of a linguistic expression is replaced by the notion of its use.

> Think of the tools in a tool-box: there is a hammer, pliers, a saw, a screwdriver, a rule, a glue-pot, glue, nails and screws.—The functions of words are as diverse as the functions of these objects. (And in both cases there are similarities.) (11)

And again,

> It is like looking into the cabin of a locomotive. We see handles all looking more or less alike. (Naturally, since they are all supposed to be handled.) But one is the handle of a crank which can be moved continuously (it regulates the opening of a valve); another is the handle of a switch, which has only two effective positions, it is either off or on; a third is the handle of a brake-lever, the harder one pulls on it, the harder it brakes; a fourth the handle of a pump: it has an effect only so long as it is moved to and fro. (12)

These comparisons help to explain Wittgenstein's comment that "to imagine a language means to imagine a form of life" (19). Asking and answering questions, describing a room, cursing, complaining of pains, expressing fears or doubts, proving a theorem in geometry, all of these are language games or forms of life in which various portions of our language are woven into activities aimed at some goal. So the notion of meaning is, for Wittgenstein, largely replaced by the notion of use. "For a large class of cases—though not for all—in which we employ the term 'meaning' it can be defined thus: the meaning of a word is its use in the language" (43). Once the identification of meaning and use is made, it becomes possible to understand some very fertile sources of philosophical confusions. The meaning of an expression is the use (or uses) that it has in some actual language game (or games). If, now, we ignore the language games which give our words their significance and regard them apart from these contexts we become puzzled as to how obvious kinds of statements such as "I know how you feel" or "I know he is in great pain" can be known to be true or even intelligible. It is part of Wittgenstein's technique to remind us constantly of the normal language games which are the homes of these statements; to remind us that in certain surroundings we do know how to use them. In one remark Wittgenstein considers the following philosophical protest: "Yes, but there is *something*

there all the same accompanying my cry of pain. And it is on account of that that I utter it. And this something is what is important—and frightful." And Wittgenstein responds with a move typical of the method of the *Philosophical Investigations*. He asks: "Only whom are we informing of this? And on what occasion?" (296). The effect of this question is to shock us out of the mood which produced the philosophical protest by reminding us that we are puzzled because we have removed our language from the normal setting which gives it meaning. The gears of the machine of language are disengaged and the engine idles. This idling of language is characteristic of philosophical talk.

Though in certain moods (philosophical ones) it can seem utterly baffling that we should be able to say such things as "I know how he feels," the puzzle never arises in any living situation in which these words are used but only when they are separated from these surroundings ("forms of life") and contemplated apart from any language game. "But what are these words to be used for now? The language game in which they are to be applied is missing" (96). The same view of the nature of philosophy is expressed when Wittgenstein says: "Philosophy begins when language goes on holiday" (38).

Here we come back again to the criticism of the private inaccessible exemplar which the private language theory requires our sensation to be. At one point Wittgenstein considers the following philosophical contention:

> Imagine a person whose memory could not retain *what* the word "pain" meant —so that he constantly called different things by that name—but nevertheless used the word in a way fitting in with the usual symptoms and presuppositions of pain. (271)

Such a man uses the word "pain" in the same way that the rest of us do, and Wittgenstein's comment on this case is this, "Here I should like to say: a wheel that can be turned though nothing else moves with it, is not part of the mechanism" (271). The private sensation is not engaged with the rest of the mechanism of our language. It is an idle wheel. We are reminded here of the pragmatist's dictum that the difference which makes no difference is not a difference. The point is brought home in the striking image of the beetle in the box.

> Now someone tells me that *he* knows what pain is only from his own case!—Suppose everyone had a box with something in it: we call it a "beetle." No one can look into anyone else's box, and everyone says he knows what a beetle is only by looking at *his* beetle.—Here it would be quite possible for everyone to have something different in his box. One might even imagine such a thing constantly changing.—But suppose the word "beetle" had a use in these

people's language? —If so it would not be used as the name of a thing. The thing in the box has no place in the language-game at all; not even as a *something:* for the box might even be empty.—No, one can "divide through" by the thing in the box; it cancels out, whatever it is. (293)

Here again a contrast is invited with the earlier *Tractatus.* There Wittgenstein characterized philosophy in this well known passage: "Most propositions and questions, that have been written about philosophical matters, are not false, but senseless" (4.003). In his later *Investigations,* the terms "senseless" or "meaningless" are carefully avoided in the descriptions of philosophical theories. Instead of such wholesale treatments we get detailed descriptions of the manner in which philosophers use their language, and this is contrasted with the ordinary uses in connection with which our language acquires its meaning.

But are not these views about the mind simply behaviorism? "But doesn't what you say come to this: that there is no pain, for example, without pain-behavior?" (281). And Wittgenstein's reply is, "It comes to this: only of a living human being and what resembles (behaves like) a living human being can one say: it has sensations; it sees; is blind; hears; is deaf; is conscious or unconscious" (281). Well, how do we know that plants or rocks can't feel and that machines can't think? It isn't because we have investigated them so carefully. Certainly we haven't investigated all of them! No, it is because it is only in such human and animal surroundings that we are taught to use words like "sensation," "pain," "see." Aside from such contexts, they simply don't apply, for the criteria of their application are missing.

The topic we are dealing with carries us directly into Wittgenstein's use of the notion of criteria. The very fact that language is used and that others can be taught to use it correctly shows that there are criteria of correctness. It is in order to remind us of the criteria of application for the various parts of our language that Wittgenstein constantly tries to get us to look at the situations in which we might learn or teach to another certain language games. "How would you teach the use of such an expression?" now comes to stand beside the earlier question, "How would you verify?" In answering either we refer to criteria. And criteria must be public. How else could we share a common language? The criterion for pain is pain-behavior, at least the criterion for others' being in pain. If we are to talk about the inner at all, the criteria must be outer manifestations. "An 'inner process' stands in need of outward criteria" (580). And, of course, the concepts of the "inner," the "private," the "inaccessible" themselves belong to our common (public) language and have in their normal language

games (public) criteria for their use. So we begin to see where those philosophers who have proposed an "ego-centric predicament" have gone wrong. For them, each of us begins with his own private language of the mind and then the predicament is as to how we can go from this privacy to a common public language shared by a community. Of course, beginning with the dogma of privacy the passage cannot be made.

In answering the question "How would you teach the language of pain?" we are led to answer the further question "What would show that the language has been learned?" And in answering this we answer the question "What features of a situation *count* as settling the issue as to whether or not these words apply here?" and notice, something must count if learning is to take place. A principal difficulty about the notion of criteria lies in the temptation to confuse it with the notion of a symptom.

> The fluctuation in grammar between criteria and symptoms makes it look as if there were nothing at all but symptoms. We say, for example: "Experience teaches that there is rain when the barometer falls, but it also teaches that there is rain when we have certain sensations of wet and cold, or such-and-such visual impressions." In defense of this one says that these sense-impressions can deceive us. But here one fails to reflect that the fact that the false appearance is precisely one of rain is founded on a definition. (354)

If we are deceived, we are deceived about rain and not something else, consequently something counts as being rain; not everything can be at most a sign or symptom of rain. This thought is continued in the next remark. "The point here is not that our sense-impressions might lie, but that we understand their language" (355).

In the case of pain-behavior and pain, the failure to keep clearly in mind the distinction between symptom and criteria may mislead us into thinking that somehow the pain-behavior is a symptom of pain, while the pain itself is hidden and inaccessible to us. It is important to remember that we have learned the concept "pain" in certain human surroundings (people holding parts of their bodies, crying, contorting). That is why we are so sure that a machine cannot experience pain. "But surely a machine cannot think!—Is that an empirical statement? No. We only say of a human being and what is like one that it thinks. We also say it of dolls and no doubt of ghosts too. Look at the word 'to think' as a tool" (360).

But surely there can be pain behavior without pain. People simulate, act, or otherwise pretend. Of course, but first of all it is to be noted that it would be impossible to pretend being in pain unless something counted as being in pain. The possibility of pre-

tending, simulating, depends upon the existence of criteria for the genuine case, and of course, there are criteria for the faked case as well. We are not saying that it is not possible to have pain-behavior without pain, but that in certain surroundings to doubt or question the presence of pain when confronted with the pain behavior is to have the very meaning of "pain" crumble away before us.

LEONARD LINSKY

UNIVERSITY OF ILLINOIS

PROFESSOR GOODMAN'S *FACT, FICTION, & FORECAST* [1]

PROFESSOR Goodman's book begins with a reprint of the essay on counterfactuals which first appeared in this JOURNAL in 1947, and continues with three lectures dealing with the same problem and certain close associates. The lectures were originally presented at the University of London in 1953, but have been revised and supplemented with notes. (These notes are almost indispensible to a complete picture of the author's position and should not be overlooked.) I shall begin by sketching in bare outline what he has to say, putting most of the emphasis on the links which are established between the various topics.

The original counterfactual problem concerns statements to the effect that if A were the case then C would be the case, where A and C are both false statements.[2] The proposed solution demands a set of statements S and a natural law L such that C is deducible from A.S together with L; and the difficulties hinge on the fact that (1) not every true S is acceptable and (2) not every true generality can count as a law. Various occasions for restricting the members of S are examined but only one is singled out as presenting a really serious difficulty (p. 23; p. 31); A and S must be "cotenable," and this condition is said to fail when S would not be true if A were true, otherwise to hold. Since this definition itself involves a counterfactual it is circular from the point of view of the problem at hand.

Cotenability is discussed on the provisional assumption that we can recognize laws as distinguished from general statements which are true by mere accident (e.g., "Everyone in this room has a

[1] Nelson Goodman, *Fact, Fiction, & Forecast* (Cambridge, Mass.: Harvard University Press, 1955), 126 pp., $3.00.

[2] There seems to be no point in introducing the generalized variant "If anything were an S it would be a P," in which the antecedent and consequent would be open statements and neither true nor false. Goodman does not in fact discuss this.

younger brother''). For the most part, the accidental kind of generalization mentions some particular object or event, and in the original counterfactual article it was suggested that a statement the confirmation of which depends on some particular instance (e.g., ''Jones is in this room and has a younger brother'') should be excluded as unlawlike. That solution, however, is blocked because it presupposes a theory of confirmation which in turn depends on the distinction between admissible (lawlike) and non-admissible generalities. In chapter III the same problem is reformulated in terms of a distinction between positional predicates which refer to specific objects and purely qualitative ones which do not. Professor Goodman seems to concede that confirmation theory could be pushed ahead on the lines already outlined by Professor Hempel if this distinction were clarified (p. 73), but in his judgment this has not been accomplished and an entirely new approach is therefore required (p. 83).

The new approach is introduced by way of the problem of dispositions, which he thinks is simpler than that of counterfactuals, although there is admittedly a temptation to assimilate the two (p. 45). Professor Goodman first distinguishes manifest from dispositional predicates (e.g., ''bends'' and ''dissolves'' as opposed to ''flexible'' and ''soluble''), and then proposes to use the former to define the latter. Clearly ''bends'' (even if expanded to include the manifest conditions under which it occurred) would not serve to define ''flexible'' since many objects are flexible which have never been bent. What is required is a manifest predicate (or combination of such) which is ''related by law'' to the dispositional term in such a way as to ''coincide in its application'' (pp. 48–49). Admittedly this presupposes the still unclarified distinction between laws and other statements, but he thinks that, with ''luck or abundant special information'' (p. 48), we might be confident the required connection held in a given case, and could go ahead anyway. In his terminology, we would then be dealing with ''the specific problem'' (of a given disposition term) as opposed to ''the general problem'' (of laws), and he is proposing that we can bypass the general problem (the solution of which would not, in any case, tell us about particular connections between events).

The importance he attaches to this move comes out in the last chapter which reopens the question of defining ''projectible.'' It is, he contends, a ''typical problem of dispositions,'' but with this particular predicate ''the stakes are high,'' for if we can handle it ''we therby solve the general problem'' (p. 89). The steps towards a solution can conveniently be divided into two stages. He begins by showing how actual projections, that is to say hypotheses

which are adopted after an examination of some of their instances,[3] can be screened to eliminate those which contain predicates such as "grue." An object is said to be grue if it is green before a specified time t, otherwise blue. Up to time t, the evidence for applying "grue" and "green" is bound to be identical but "green" turns out to be better entrenched,[4] that is to say it has been used for projection more frequently (p. 95). A rule is therefore laid down (p. 96) which displaces "grue" in favor of "green," and two comparable (although much more complicated) rules are arranged to dispose of other fabricated predicates all of which depend on a unique time reference (pp. 101–104).

These three rules work only for hypotheses which can be paired in respect to the same evidence and are not effective in connection with the positional predicates considered in chapter I, e.g., "object in my pocket."[5] The second phase of the investigation introduces the notion of comparative projectibility which applies to these last as well as to all "normal" predicates. At this point he drops his original procedure of considering only actual projections and shifts attention to the class of presumptively projectible hypotheses. This set includes all those satisfying the conditions required for actual projection provided they also conform to the three elimination rules mentioned above.[6] In terms of the comparative concept, these will vary "all the way from the highly projectible to the virtually unprojectible" and the program calls for a precise account of these distinctions (p. 107). The leverage on this problem is provided by the possibility of classifying hypotheses in such a way as to increase or decrease the support offered by their positive instances. To use Professor Goodman's example, the projectibility of "All marbles in bag B are red" goes up if, in addition to evidence offered by red marbles in bag B, we discover that bag B comes from a stack of bags whose contents are uniform in color. Here, and throughout the book, marbles, variously distributed,[7] assume the vicarious role of natural events which can be described by

[3] The precise definition of "actual projection" appears on pp. 90 and 92.

[4] The notion of entrenchment, which initially refers to predicates of objects, is later enlarged to include certain "parent predicates" which serve to classify ordinary predicates. Under favorable circumstances, the latter "inherit" entrenchment from the former. See pp. 105–106.

[5] Probably these could be paired in some fashion but this move appears unprofitable and it is not suggested in the text.

[6] I construe the definition on the middle of p. 108 in such a way as to admit actual projections into the class of presumptively projected hypotheses. The phrase which appears at the top of the page in reference to what "could have been projected" conflicts with this.

[7] For the most part, they are found in bags which, in turn, appear in stacks located at one point or another in the state of Utah.

singular statements and laws. The upshot is that the unprojectibles which at first imposed a barrier against any theory of law whatever are now largely assimilated [8] in a wider category of lawful sentences. Lawlikeness then becomes a matter of degree and its discovery will often be "a long and difficult process" (p. 116), rather than a character which is assigned definitely before confirming evidence is allowed to count at all. Nevertheless, what has been done offers a way of dissolving the difficulties associated with counterfactuals, dispositions, etc., and the crux of the solution, in Professor Goodman's estimation, is linguistic. "If I am at all correct," he says, "the roots of inductive validity are to be found in our use of language"; and again, "the line between valid and invalid predictions (or inductions or projections) is drawn upon the basis of how the world is and has been described and anticipated in words" (p. 117).

Anyone who takes a careful look at this book is unlikely to expect even the most sympathetic critic to touch on more than a fraction of the points he raises. The sketch I have just given is intended to provide a thread which ties together certain basic assumptions on which his case rests. For the most part, as the reader will discover, I consider these assumptions debatable but I hope these queries, even if misguided, will make it easier to see what is going on. I shall begin with one of the major difficulties in the analysis of counterfactuals, the demand that the assumptions involved must be cotenable as well as compatible with the antecedent.

Professor Goodman's exposition of the cotenability problem is quite brief considering that he regards it as particularly crucial and only hints at a solution in the last chapter. An example of a radiator which is assumed, contrary to fact, to freeze but never to reach a temperature below 33° F. is used (p. 18) to illustrate the requirement of physical compatibilty (he usually refers to it as non-logical compatibilty). There are no such radiators, so an A.S combination (antecedent plus further assumptions) which describes one will lead to paradoxical results by way of a vacuous law. It is then pointed out that the standard example of a legitimate counterfactual, "If this match had been struck it would have lighted," can be modified to give "If this match had been struck it would not have been dry" by including in S the (true) statement that the match did not light. Here he accepts S as both logically and physically compatible with A, but maintains that it

[8] I deliberately use the phrase "largely assimilated" for I do not believe that Professor Goodman goes quite far enough along the lines just sketched. Compare my remarks towards the end of this article.

"would not be true if A were true" and is thus to be ruled out by the cotenability requirement.

No other example of cotenability is given, and as I am not convinced that either this or the radiator case is altogether typical of counterfactual reasoning, I would like to mention another pair which might contribute to an intuitive grasp of the problem. (These will also be useful in evaluating the current proposals to replace counterfactuals by prediction schemata which I will discuss later on.) Suppose that a gas, at a specified temperature T, is confined in such a way that its volume could be altered without exchange of heat with the surrounding environment. Suppose further that a counterfactual, to the effect that if the volume were halved the pressure would have been doubled, is defended by reference to Boyle's law. Here the antecedent A will describe the reduction in volume, S will describe the conditions under which the gas is confined (initial temperature is T, perfect insulation, etc.) and will include a statement to the effect that the temperature remains constant. It is possible, however, to predict that under these conditions temperature as well as pressure will increase[9] so that S together with A forms a physically incompatible set and this counterfactual must be rejected. It could, of course, be retained if we modified our situation in such a way that heat was exchanged with the surroundings so that an approximate constancy of temperature could be predicted. (Under these circumstances an incorrect counterfactual would be obtained if it were assumed that temperature rose as volume was reduced.) These examples are intended to suggest that the characteristic mistakes of counterfactual reasoning depend on miscalculating what remains constant and what changes in relation to the change described by the antecedent. The half facetious rejoinder to an assertion such as "If x had been smarter he would have understood y's course" is evidence that this peculiarity is well recognized. "If x had been smarter," so runs the reply, "he would not have taken y's course." Professor Goodman's two examples are not well chosen in this respect since they involve no errors in prediction which anyone would be likely to make. The radiator case ("If that radiator had frozen and not reached a temperature below 33° then it would not have broken") depends on using a vacuous law or a fabricated variant of a vacuous law (pp. 18–19). The match case ("If that match had been struck it would not have been dry") is out of line because the information we should normally use (about the conditions under which the match was struck) becomes inaccessible.

[9] Intuitively the prediction is easy, technically it offers considerable difficulty.

Suppose the example of the match was handled along the same lines as the gas example above. The antecedent A will describe the striking of the match, S will describe the conditions under which it is struck (it is dry, oxygen is present, etc.) and will include a statement to the effect that it did not light. It is possible, however, to predict that under these circumstances the match will remain dry and light, so that S together with A forms a physically incompatible set and this counterfactual must also be rejected. Notice that neither here, nor in the example of the gas, is any reference made to cotenability. I consider that this is a sensible way to handle the situation, but it remains to show why it is not available to Professor Goodman. The answer hinges on my inclusion in S of a statement to the effect that the match was initially dry. He, on the other hand, takes no account of time relations, and the inclusion of such a statement in S would (for him) be equivalent to denying the consequent ("the match was dry") which leads to paradoxical results (as he explains on p. 20). In effect, I have thrown the description of laws and events into the ordinary causal pattern; at the beginning of a short time interval the match is dry, oxygen is present, etc., and the match is struck; these conditions remain substantially unaltered and during the last part of the interval the match accordingly lights.

The fact that he ignores this framework can hardly be explained in such terms as "inattention to customary procedures" since the whole thing is obvious. The more likely explanation is just the reverse, for every well educated philosopher has learned (correctly) that the common-sense causal pattern is a rough and often misleading sketch. You cannot (so the story goes) make any plausible statement about how long the interval must be, just how the events are dated, and how many changes in the initial conditions will occur; at any rate, you cannot do so and expect to come out with laws which are anything more than approximations. This way of thinking is therefore unavailable to a precise analyst. Nevertheless, I think this causal framework is the background against which ordinary counterfactuals are actually asserted and, which is more important, that the cotenability problem could not otherwise arise. As evidence for this statement the reader might try the experiment of examining a situation which can be described without reference to time distinctions. Imagine a set of marbles with surfaces divided into regularly numbered spaces which are variously colored. Let a given marble be red, white, blue, and green in spaces 1, 2, 3, and 4, respectively, and consider the counterfactual "If it were yellow in space 1 it would be pink in space 4." Our "laws" in this case will be generalizations describing the association of colors;

and we may discover that any marble which is yellow, white, and blue in spaces 1, 2, and 3, respectively, is invariably pink in space 4, so the above counterfactual would be supported. All the rules which have been set up to restrict counterfactuals in respect to the logical and physical compatibility of the antecedent A, the additional assumptions S, and the consequent C, can be exemplified in this model. If, for example, it so happened that no marble which is yellow in space 1 is also white in space 2, the A.S combination used above would have to be excluded as physically incompatible. On the other hand, it appears impossible to construct in any timeless model an analogue of cotenability as something distinct from logical or physical compatibility.

In his final pages Professor Goodman does hint at a solution of the cotenability problem which is relevant to what has just been said. He has explained that the incorrect counterfactual "If that penny (also) had been put in my pocket on VE day, it (also) would have been silver on VE day" can be eliminated by the conflicting semifactual [10] "If that penny (also) had been put in my pocket on VE day, it would have remained copper on VE day," since the latter is supported by a stronger law (p. 118). "Along these lines," he adds, "we may well be able to account for the falsity of such a counterfactual as 'If that match had been struck it would not have been dry,' and so to answer even the most stubborn remaining question concerning counterfactual conditionals" (p. 119). Neither the required semi-factual nor the law is set down, but if we stick closely to his analogy the latter should be "Dry matches which are struck (other conditions being normal) remain dry." Here the phrase "remain dry" would concede the importance of the time sequence in the analysis of the situation.[11]

I have said a good deal about cotenability, not only because I want to make my own criticism clear, but also because I believe that Professor Goodman has taken very little trouble to explain this topic.[12] Concerning the distinction between positional and qualita-

[10] Recall that a semifactual is understood to negate a counterfactual (p. 15).

[11] Even if cotenability should continue to be accepted as a problem, I am not convinced that this solution of the match case will stand up. The law which supports the objectionable counterfactual is (I presume) "Matches which are struck and don't light (other conditions being normal) are not dry." If Professor Goodman's answer is correct, this will have to be weaker than "Dry matches which are struck (other conditions being normal) remain dry"; but this does not look very plausible to me. The reader should consult chapter IV for his theory of how relative strength of laws is determined. I shall not return to this particular point.

[12] In a note to the first of the London Lectures (note 5 to p. 42) he says "The problem of the relevant conditions—most acutely felt as the problem of

tive predicates, which was the original stumbling block to a separation of laws from accidental generalizations, I have only one comment to make. Professor Goodman, of course, reappraises the whole matter (in chapter IV); but at first he took it to be crucial, and my question is why did he need to do so. To me, the puzzle does not seem to be very urgent and he himself is often willing to accept distinctions which are equally difficult to make precise. The nub of the issue is illustrated by supposing that we have two languages with the following peculiarity. In L_1, "grue" is defined as "green before time *t*, otherwise blue" and "bleen" as "blue before time *t*, otherwise green"; in L_2, the procedure is reversed and "green" is defined as "grue before time *t*, otherwise bleen" and "blue" as "bleen before time *t*, otherwise grue." From a purely syntactical point of view, "grue" and "bleen" can be recognized as positional in L_1 (time designations always refer to particular objects or events) while in L_2, "green" and "blue" are positional (p. 79). The question now is which of these are "really" positional, and this, according to Professor Goodman, can't be answered within syntax (p. 83). To this I agree, but as far as I can see it cannot even be stated in syntax, for in order for the positional "green" in L_2 to damage the innocent (qualitative) "green" in L_1 it is necessary to suppose that the latter can be correctly interpreted by means of the former.[13]

For a problem of this sort to be really pressing it is also necessary to persuade oneself that such conflicts actually occur between natural languages and I have seen no evidence produced which indicates that this is the case. For this reason, I would expect anyone who had a stake in confirmation theory, and confidence in the program which Professor Hempel was advancing in the 1940's, to accept a definition of positional predicates which was relative to a given language. Professor Goodman is quite willing to go ahead with a relativized definition of manifest as opposed to dispositional predicates and the distinction is, if anything, more obscure. I imagine that he does this not on the basis of a sharp decision as to how much clarity is indispensible, but rather on a kind of wager as to what line of investigation will pay off in the long run. (With this move I would be entirely in sympathy.)

Now granting there is some appropriate way of distinguishing

cotenability—does not, as some writers seem to suppose, reduce in any ready and obvious way to the problem of law." As a rule he takes some pains to lead us up various blind alleys and then bring us back on the right track. In this instance we are not told what kind of case the above mentioned writers made or how it was demolished. My objections may have already been answered.

[13] The notion of correct interpretation is by no means transparently clear.

manifest predicates, I still have a fairly simple difficulty with the thesis which Professor Goodman defends in chapter II. If what he says is taken literally, we define a disposition term by selecting one or more manifest predicates which are (or are at least believed to be) connected with it by law so that the extension of the two coincides (pp. 48–49). He points out (note 9 to page 48) the use of words which refer to "the micro-cosmic structure of things" is question-begging in an enterprise of this kind, for these will themselves be dispositional. Any of the technical terms of science would, I presume, be in the same boat, for he has explained (p. 44) that even the most commonplace predicates which refer to the enduring characteristics of things are dispositional. At this point one's natural inclination is to conclude that the required definitions are impossible. It is not easy to find invariable connections among the enduring characteristics described by common sense; if these, along with the technicalities of science, are ruled out the chances of getting results with manifest predicates seem slim.

The difficulty is not cleared up by the single example which Professor Goodman introduces in this chapter, namely, the exhibition of a given spectroscopic pattern as a sign of flexibility (p. 50). Ostensibly, this employs technical scientific language, and if one were to reconstrue it to mean, let us say, exhibition of a specified visual pattern under apparent spectroscopic examination, the alleged regularity of its connection with flexibility would be thrown into doubt. But this example was presented in connection with what the author refers to as a "digression on the desirability of defining dispositional predicates" (p. 50). The intent of the digression is to show that if a term is introduced by reduction sentences in such a way that it only has meaning in a limited context, it would be just as easy to invent a new term restricted to just this context. Such terms could be defined in the strict sense and complete elimination would always be possible. Thus, to elaborate his example, we could introduce a term such as "spectro-bends" which will apply to a given object x if x is either (1) under actual pressure and bends, or (2) under actual spectroscopic examination and exhibits the pattern referred to. I am willing to concede that this could be done although I am not convinced that it would offer any substantial advantage over reduction sentences. (I am not, in any case, favorable to reduction sentences when they are used in this way to give a piecemeal criterion of meaning.)

But if the specialized definition procedure just described is what Professor Goodman had in mind throughout the chapter, the original suggestion that we look for manifest predicates which coincide with ordinary dispositional ones should have been revised; the

required coincidence is with the more specialized disposition terms like "spectrobends." My guess is that the specialized kind of definition was, in fact, what he had in mind. If this is the case my difficulties move over to chapter IV where "projectible" is treated as a typical disposition predicate; there is no suggestion that the specialized definition procedure is being used in these pages. However, I have more basic objections to treating "projectible" in this way and, if they are substantiated, the point can be dropped.[14]

The classification of "projectible" as a typical disposition term seems to me to be dubious. Compare "provable," which characterizes an expression in a formal system when there is a proof connecting it with the axioms, a proof in turn being defined as a sequence of formulas satisfying specified conditions. "Theorem" would be defined in just the same way, which suggests that the suffix "able" is a somewhat accidental accretion, perhaps due to the habit of focusing on the personal manipulation of symbols.[15] Under certain circumstances, relating to the ingenuity of the individual and the character of the system, the provable formula might be regarded as generating a proof much as a flammable substance generates a flame. The same considerations apply in reverse to "well-formed" as a characterization of formulas. Here the suggestive suffix is missing but we might be led to apply the term "admissible" simply by focusing on the possibility of generating these expressions from the primitives by constructing suitable sequences. But there seems to be no very compelling reason to adopt this slant and it is out of line with the usual point of view which regards logic as a formal science. I am suggesting that other methodological terms outside the formal sciences have the same character. They set down qualifying conditions which sentences have to meet in order to be classified as, for example, laws, observations, statements, supporting evidence for other sentences, etc. A sentence is "projectible" in Professor Goodman's original usage of the term when it has instances which qualify as evidence for it, and there is a broader usage which he eventually adopts in which a wide variety of evidence determines a comparative concept "degree of projectibility." Just how the qualifying conditions for

[14] It should be noted, in passing, that almost half of chapter II is devoted to explaining how possibles (in a certain sense, e.g., possible wars) can be treated in terms of dispositions. I think this is technically correct but the importance he attaches to it hinges on the success of his definitional procedure.

[15] The suffix is not supposed to be a necessary feature of disposition terms as Professor Goodman is careful to point out; "hard," for example, is cited as dispositional (p. 44).

the wider concept should be laid down is debatable, but there seems to be no reason to approach it as a disposition term.

There are two fairly simple considerations which support this contention. In the first place, we never withhold an ordinary disposition term such as "soluble" when the corresponding manifest predicate (the one which Professor Goodman describes as being "projected") has actually been exhibited. Objects which have never dissolved may not be soluble but objects which have dissolved cannot be denied this character, at least not unless we are specializing the predicate in one way or another. (Objects which dissolve in carbon tetrachloride may not be soluble in water; substances which are fluid under enormous pressure may not be considered as fluid if we intend the term to refer to "normal" conditions.) On the other hand Professor Goodman admits that hypotheses may be actually projected without being projectible. "We are speaking here," he says in discussing actual projection, "not of what ought to be projected but of what is in fact projected" (p. 90). The other point concerns the way in which chapter IV ("Prospects for a Theory of Projection") is organized. If two or three references to the "problem of projecting the predicate 'projected' " were deleted, this could be read as an ordinary explication [16] without reference to the supposedly dispositional character of the term.[17] The deletions would, as far as I can see, make no difference to the effectiveness of the argument.

If my objection to treating "projectible" as a disposition term is correct, it applies more to what is said than to what is done. It is worthwhile to raise the point for the sake of being right (or at least raising the query as to who is right) but I am not proposing that Professor Goodman's enterprise should be stalled on this issue. I have a comparable, but more serious, difficulty in respect to his thesis that the distinction between valid and invalid projection is a function of linguistic practice. In what I have referred to as the first phase of his theory the description is no doubt justified. Here he deals with actually projected hypotheses and matches certain pairs in order to eliminate those involving poorly entrenched predicates. The advantage of such an approach by comparison with what was done in the earlier chapters stands out very clearly. There it was necessary to refer to laws, and these were understood to be general statements confirmed by their positive instances.

[16] I am using "explication," after the fashion of Professor Carnap, as a procedure which leads to a definition fulfilling certain requirements.

[17] The required deletions are: (1) the top paragraph of p. 89 beginning "Nevertheless this slight reorientation . . ."; (2) lines 24–27 on p. 93; (3) the last clause of p. 93 beginning ". . . the twofold problems."

But difficulties arose in deciding which general statements were so confirmed and these difficulties, as we have seen, were not overcome to Professor Goodman's satisfaction.[18] The new attack begins by considering any and all hypotheses which are actually adopted (projected) without requiring any preliminary sifting. Hypotheses containing fabricated predicates are made to compete with those involving a normal vocabulary, the requirements of competition being that the same evidence is relevant to both. The three rules of selection are not all simple[19] but the end result is pretty well assured; in every case predicates which are little used are displaced in favor of those that are well established (entrenched). This kind of sifting does depend on usage and here we have solid support for his concluding remark that the line between valid and invalid projection depends on "how the world has been described and anticipated in words."

But the first phase of the theory by no means covers everything that has to be done, for only a small class of hypotheses are open to the sort of pairwise comparison which is required. The whole group of positional predicates introduced in chapter I (pp. 25–29) such as "coin in my pocket," "person in this lecture audience," etc., are inaccessible to the three elimination rules, and "normal" predicates such as "red," "conducts electricity," etc., are clearly out of bounds. Thus the solid work, once the notion of comparative projectibility is introduced, will have to be centered elsewhere. But once it becomes apparent that comparative projectibility does most of the job, it is not unnatural to consider the possibility of letting it do the whole job, and this has not escaped Professor Goodman. "A natural question," he says (p. 116), "is whether we might not bypass the rules of elimination and the definition of presumptive projectibility entirely, begin by assigning initial indices to all hypotheses, and rely on eliminating all invalid hypotheses by the principle that a hypothesis is to be given up in favor of any more projectible competing hypothesis, and the principle that any hypothesis with a negligible degree of projectibility is to be summarily dismissed." In regard to the rules of elimination this suggestion seems excellent, and the more so (in my opinion) in that it would allow us to drop the consideration of entrenchment altogether.[20] (For reasons which I will explain shortly, I think

[18] Compare his summary remarks on "the new riddle of induction" (p. 81).

[19] These rules appear on p. 96, p. 99, and p. 104, respectively.

[20] I have said very little about entrenchment because of the maze of complications which it raises. Here, the most relevant difficulty concerns the coördination of this concept with that of comparative projectibility. "Bagful

the practice of considering only those hypotheses which have some positive support and which are not yet exhausted should be retained.) Professor Goodman is unfavorable to this choice, partly because he looks favorably on the elimination rules which he characterizes as "crisp," and partly because he finds the concept of degree of projectibility to be involved and delicate. This may be the case, but he has to use it anyway; if it works in most areas it should work in this one.

There is one peripheral consideration which counts in favor of dropping the three elimination rules. It is a desirable feature of Professor Goodman's new approach that it permits a less dogmatic attitude towards what is and is not lawlike. Even a sentence such as "All coins in my pocket are silver" could, in theory, turn out to be admissible (p. 118). Now this is a sentence which is positional in the spatial sense, and there is certainly a kind of justice in admitting it at least as a candidate for respectibility; ancient astronomy, not to mention that of Copernicus, was positional in just this way.[21] The three rules, on the other hand, eliminate certain temporally positional predicates without more ado. Why the distinction? You could point out that "grue" and "grund" are absurd, but there would be no more difficulty in getting rid of them by way of the comparative concept than is involved in disposing of "coin in my pocket."

I remarked above that even if the three elimination rules were cut out it would be advisable to retain the practice of operating only on hypotheses for which there is supporting evidence and which are neither exhausted nor violated.[22] The reasons for saying this are quite simple. It automatically provides a way of eliminating vacuous laws as well as those which have been deliberately constructed to apply to a limited number of individuals whose characteristics have already been examined. Most philosophers propose to dispense with these, and there appears to be no simpler way of doing so. Moving ahead on these lines, the landmarks now become more familiar; in fact the concept of comparative projectibility has a close family resemblance to that of degree of confirmation when this is handled (as was once customary) by way of the amount and kind of supporting evidence. The cases

of marbles" is said to be exceptionally well entrenched (p. 106); presumably "test-tubeful of marbles" is not. But this last might be highly projectible in terms of the comparative concept. How make a joint evaluation?

[21] Nothing is said in the lectures about hypotheses which are positional but neverthless derivative from more general laws. There is no reason to suppose they could not be fitted in.

[22] Compare the definition of "presumptively projectible" on p. 108.

he takes up all follow the pattern which has sometimes been described as a search for natural kinds, that is to say identification of classes the members of which exhibit a kind of uniformity (say, in respect to color or electrical conductivity or melting point). For the most part they are token examples and concern marbles in bags and bags of marbles in stacks, etc. (pp. 109–115), but the analogy with such things as ravens and pure copper is obvious enough. The unobtrusive way in which the required concepts are introduced forms an interesting contrast to Professor Goodman's earlier chapters, where it was scarcely possible to state a problem without running into insuperable difficulties. As might be expected, many of these lie outside the original notion of lawlikeness which was introduced in connection with the counterfactual problem. For example, "support," which depends on the number of confirming instances, is sharply distinguished from "degree of projectibility" as a function of certain background hypotheses, and both of these contribute to the notion of "effectiveness"—which, to the best of my knowledge, is never officially defined (pp. 114–115). This broadening of the original notion of lawlikeness into the new concept of comparative projectibility is unobjectionable in principle but we would naturally expect that the latter would do all the work of the former. At one point, however, it appears to fail. Professor Goodman provides the example which, curiously enough, is the case first cited in chapter I as a typically unlawlike sentence, "All coins put in my pocket on VE day were silver on VE day" (p. 118). Suppose, on the basis of reinforcing evidence such as "All coins put into any pocket on any given day are uniform in composition," that this sentence achieves a high degree of projectibility and suppose, in addition, that it is true. According to Professor Goodman, it would now have the status of a law, and the uncongenial counterfactual, "If that penny (also) had been put in my pocket on VE day, it (also) would have been silver on VE day," would be fully supported. (To all appearances we would be back where we started from.) I have already referred to his solution, which was to admit the law but discard the counterfactual on the basis of conflict with a semi-factual, which is more strongly supported by the law "Coins remain of the same material regardless of (mere) changes of place." But why not dispense with the weaker law in the first place? The answer, I presume, is that the two generalizations conflict counterfactually but not logically (there were no copper coins put in my pocket on VE day), so that both remain true and must be counted as laws.

If this is the correct diagnosis, I think Professor Goodman is ignoring the advice which he gave in chapter III to the effect that

principles of inductive inference should be justified by inductive practice (p. 67). Conflicts of this kind have more claim to consideration than those associated with "grue" and "green," yet he was quite willing to use the latter as a screening device which would precede the application of all the criteria for comparative projectibility (p. 108). A comparable procedure is available here, although it would be harder to describe, merely because the screening would have to be introduced piecemeal as new evidence accumulated.[23] With this slight revision, Professor Goodman's theory seems quite successful in handling the problem of lawlikeness and, in contrast to the rigid exclusion of positional predicates, it has much to recommend it on intuitive grounds. Although I would be inclined to consider this as the chief accomplishment of the book, he puts the emphasis elsewhere. He admits that the considerations he brings to bear on degree of confirmation are not novel,[24] since background hypotheses ("overhypotheses" in his technical terminology) have always been recognized as making a difference. What he says has been overlooked is "the cardinal fact" that these must also be lawlike (p. 116). It is, of course, true that unlawlike hypotheses which involve manufactured predicates have been overlooked at all levels. A kind of unconscious wisdom seems to have sifted them out as an unprofitable investment. If Professor Goodman is on the right track, this is well justified since their index of projectibility will be low enough to eliminate them from consideration. The more difficult problems will come up if we try to deal with background hypotheses which have somewhat higher indices of lawlikeness, and here I think it would be fairer to say that nothing much has been done rather than that any special factor has been disregarded.[25] He himself offers only a few suggestions and some

[23] It is not necessary to have a complete theory of counterfactuals in order to define the relevant conflict between pairs of laws. We will call this "hypothetical conflict" and say that two laws L_1 and L_2 conflict hypothetically when there is a statement S such that (1) S is logically consistent, (2) S is physically consistent provided either L_1 or L_2 is canceled, (3) S is physically inconsistent if both L_1 and L_2 are counted as laws. ("Physical consistency" is to be understood in the same terms as "physical compatibility," which was used earlier.)

[24] The references are on pp. 115–116. Earlier (p. 88) he speaks of "a long standing habit" of regarding the successes and failures of past predictions as irrelevant; this, I believe, refers to a different group of investigators.

[25] A number of authors currently refer to the selection of background hypotheses to reinforce ("increase the projectibility of") ordinary hypotheses as inductive policy. One of the oldest and most plausible policies is what Whewell called "concilience" which, in effect, upgrades those laws which can be derived from a well established theory. If the reader will try to make a check list of recognized inductive policies, he will see why I said that nothing much had been done.

of these point towards the field of statistical theory where this sort of thing has not been overlooked.[26] Consequently I feel that there is not enough evidence on hand to justify any further criticism along these lines.

There are a variety of ways in which counterfactuals can be construed. Professor Goodman explains that he is not considering a number of these, as for example counteridenticals, and other types which have subsequently been mentioned in the literature would clearly be out of bounds.[27] I have taken pains to observe these limitations and to discuss only the questions which he has raised, but I would like to take up one suggestion which has been made by Professors Chisholm, Hiz, and others.[28] This consists in construing a counterfactual as an oblique way of indicating what can be deduced when the antecedent is taken as a supposition and certain other statements are presupposed. The presuppositions are not made explicit in the counterfactual version itself, and it is the special feature of this point of view that we withhold judgment until this is done. Rather than trying to fit a counterfactual to a general rule which determines whether or not it is true, we simply replace it by what might be called a prediction schema. In a prediction schema, all the supposedly relevant premises together with the conclusion are set down. There will, of course, be many of such schemata corresponding to any given counterfactual and each will be judged on its merits when and if it is brought up for consideration.

The phrase "prediction schema" is one which I have improvised myself; "supposition" and "presupposition" are taken over from Professor Chisholm. In what follows, I shall not attempt to reproduce his point of view exactly but will rather present one which resembles it in outline and which will, I hope, throw some light on what has already been discussed. I shall let the presuppositions of a schema correspond to Professor Goodman's set S, together with any relevant laws. (I am not perfectly sure that Professor Chisholm would assent to this.) Also, I will leave open the question of whether schemata must always be judged individually. Granting all this the reader will probably ask why the Goodman criterion should not be applied to prediction schemata, thus making them acceptable only if the supposition and presuppositions are

[26] Statistical overhypotheses are mentioned on p. 114; the problem of randomness is referred to on p. 119 as amenable to treatment within his theory.

[27] As an example, take the sentence "If he were guilty he would show just the signs of excitement which he does show."

[28] R. Chisholm, *Analysis*, April, 1955; H. Hiz, *Journal of Philosophy*, 1949, pp. 586–587.

logically and physically compatible [29] and jointly lead to the conclusion, etc., etc. I think it does apply, but that the chief virtue of the new approach consists in pointing up the irrelevance of applying it to the more common difficulties associated with counterfactual reasoning. Take a case of a kind which Professor Chisholm discusses in considerable detail and which leads to a counterfactual of the form "If x had been elected two years previously, our foreign policy today would be so and so." [30] It is easy to recognize that a great many presuppositions are at least plausible in respect to what conditions would be substantially unaffected by the election of Mr. x, and there would be a like number of reasonable guesses as to what would change. To set these down in a prediction schema would have the virtue of exposing areas of agreement and disagreement and, for practical purposes, there seems to be no alternative approach. There is no possibility of making a list of laws and true statements, checking for compatibility, and following this up by noting certain deductive consequences, for the real difficulty is one of diagnosis; we don't know just how the situation should be analyzed and how to predict.

This is an extreme case, but there is no sharp line which divides it from that of the struck match. We cannot literally presuppose that nothing changes as a result of the striking. Air currents are set up, but not violent enough to extinguish the flame; oxygen is consumed when the reaction begins, but not enough to prevent the match from lighting; moisture is introduced, but not enough to affect the behavior of the match. Thus our presuppositions will record changes which can be ignored relative to the rough and ready law, "Matches which are struck under normal conditions will light." But here again, the diagnosis could be questioned and no doubt would be if the match supply were low and there was some importance attached to starting a fire. By explicitly laying out a prediction schema, an individual sufficiently wise in applied chemistry might correct the first appraisal, but he could only do so by bringing more expert knowledge to bear. In a more sophisticated form, the same problem is raised by the gas example which I used above. Here I assume that we want to know just

[29] From the point of view of those who are unconvinced by what I have said, they should also be cotenable.

[30] Professor Chisholm takes up a quite specific question rather than a counterfactual, asking what American foreign policy would be today if Stevenson had been elected in 1952. In shifting, here and elsewhere, between counterfactuals and questions, he appears to assume that neither can be handled until certain presuppositions are made explicit, and that for the most part there is nothing which forces us to accept one presuppostion rather than another.

how much the pressure and temperature will change when the gas is compressed under conditions which do not permit exchange of heat with the surroundings. To do so we turn to an adequate text on thermodynamics, examine the section on adiabatic changes in the volume of a gas, and calculate accordingly. Most philosophers (including the writer) would need the assistance of a physicist, but this is beside the point. The calculation could be made, and it might or might not be appropriate to the conditions described. Here again it is a diagnosis of these conditions which makes a correct result possible. To point up what has been said, assume that someone has used what I call a prediction schema on one of the cases cited (or any comparable situation), that his result is finally recognized to be incorrect, and that he asks for a rule which will prevent such mistakes in the future. Clearly there is no rule which will do just this, and, as I said before, the Goodman criterion of deducibility from compatible premises could apply without affecting this question in any way.

But I am not claiming that this criterion has no relevance to prediction schemata. Consider a schema which is applied to a certain gas the temperature of which is 60°. The supposition will be that the gas has a temperature of at least 70°; the presuppositions are, first that its temperature does not lie in the (closed) interval between 70° and 80°; and, second, that if the temperature of any gas is over 70° and does not lie in the (closed) interval between 70° and 80°, then it is over 80°. The conclusion is that its temperature is over 80°. Here we have a correct deduction from a counterfactual supposition and true presuppositions and there is no problem of diagnosis. On intuitive grounds, it is plausible to say that every correct schema will follow this pattern, in other words that it is a necessary condition. But the above example shows that it is not a sufficient condition for correctness. Translated into the language of prediction schemata, Professor Goodman's problem is to show how the pattern can be modified in such a way that it represents a sufficient as well as a necessary condition.[31] In addition, of course, he has to deal with the problem of law but I have already explained how he stands on this point.

In fairness to Professor Chisholm, I want to repeat that I do not pretend to reproduce his position exactly, but have used it rather as a springboard. What I wanted was a fairly simple statement of the point of view that counterfactuals need not be considered as true or false statements because they merely substitute for a kind of hypothetical deductive procedure. One of my

[31] For a solution of such problems, consult chapter I of *Fact, Fiction, and Forecast*.

conclusions is that this approach forces us, so to speak, to put our cards on the table in any given case, and in this way brings out what I have called the diagnostic difficulties of counterfactual reasoning. Aside from a certain specialized philosophical interest,[32] these seem to me to be the only difficulties which normally concern us. But if, on the other hand, we are able to surmount (or simply ignore) the problem of diagnosis, the specialized questions of the sort which Professor Goodman has raised will apply just as readily, whether one uses straight counterfactual sentences or prediction schemata of the kind I have described. It is true that reasons can be advanced against raising these specialized questions at all, and if they are accepted, schemata provide a better means of expressing the sense of the counterfactual. But this is simply because they require that all premises (presuppositions) be laid out and thus invite a specific criticism of each case without demanding much generality. For example, one of Professor Chisholm's reasons for shifting to prediction schemata is his conviction that any general characterization of laws would itself involve counterfactuals, and thus lead to circularity if applied to that particular problem. By treating each schema individually and not distinguishing laws from other presuppositions, this difficulty will be avoided, if it exists. My conviction is that the degree of generality involved here is not unmanageable.

JOHN C. COOLEY

COLUMBIA UNIVERSITY

NEW BOOKS

BURTT, EDWIN A.: *Man Seeks the Divine; a Study in the History and Comparison of Religions.* New York: Harper [1957]. xii, 561 p. $4.75.

CALIFORNIA, UNIVERSITY OF: *George Berkeley.* Lectures delivered before the Philosophical Union of the University of California, in honor of the two hundredth anniversary of the death of George Berkeley, Bishop of Cloyne (1685–1753). Berkeley & Los Angeles: Univ. of California Press, 1957. v, 206 p. (Univ. of California Publications in Philosophy, Vol. 29.) Paper, $4.00.

EWING, A. C.: *The Idealist Tradition, from Berkeley to Blanshard.* Ed. with an introd. and commentary by A. C. Ewing. Glencoe, Ill.: Free Press & Falcon's Wing Press [1957]. viii, 369 p. (The Library of Philosophical Movements. General Editor: Paul Edwards.) $5.50.

FRYE, NORTHROP: *Anatomy of Criticism; Four Essays.* Princeton, N. J.: Princeton Univ. Press, 1957. x, 383 p. $6.00.

GOUGH, J. W.: *The Social Contract; a Critical Study of Its Development.* 2d ed. Oxford: Clarendon Press, 1957. viii, 259 p. $4.80 (30s.)

HACKMAN, GEORGE G.; KEGLEY, CHARLES W.; NIKANDER, VILJO K.: *Religion in Modern Life.* New York: Macmillan [1957]. viii, 480 p. $4.25.

HOFMANN, JOSEPH EHRENFRIED: *The History of Mathematics.* New York: Philosophical Library [1957]. xi, 132 p. $4.75. (Tr. from the German *Geschichte der Mathematik* by Frank Gaynor and Henrietta O. Midonick.)

THE INTERNATIONAL INSTITUTE OF PHILOSOPHY
announces the publication of Vol. III of its *Quarterly Bulletin*

BIBLIOGRAPHY
OF
PHILOSOPHY

published for the International Federation of Philosophical Societies under the auspices of the International Council of Philosophy and Humanistic Studies with the aid of UNESCO and of the French National Center for Scientific Research

Provides abstracts, translated into either English or French where necessary, of books in all branches of philosophy that are published in countries in correspondence with the I.I.P.

To secure a complete file, subscriptions should begin with January, 1954. Continuation volumes of the annual bibliography for 1951 and 1952–53 will appear shortly.

Annual subscription: $3.00. Special rates to members of associations affiliated with the I.I.P. $2.75. Single copies, $1.00. Subscriptions may be sent to Professor Harold A. Larrabee, Director, Editorial Center, U. S. A., Bailey Hall 207, Union College, Schenectady, New York; or to

LIBRAIRIE PHILOSOPHIQUE J. VRIN
6, Place de la Sorbonne
Paris V°, France

Stechert-Hafner, Inc., New York – **Atlas Publ. & Distr. Co., Ltd.**, London – **Hermann & Cie.**, Paris – **Nicola Zanichelli**, Bologna – **H. Bouvier u. Co.**, Bonn a/Rh. – **J. Villegas**, Madrid – **F. Machado & Cia**, Porto – **Gerold & Co.**, Wien – **F. Rouge & Cie.**, Lausanne – **The Maruzen Company**, Tokyo.

"SCIENTIA"
INTERNATIONAL REVIEW OF SCIENTIFIC SYNTHESIS
(A REVIEW COVERING ALL SCIENCES)
(1957—51st year)

Chief-Editor: P. BONETTI

Scientific Committee: G. ABETTI – R. ALMAGIÀ – L. CALIFANO – G. COLONNETTI – A. GHIGI – F. GIORDANI – M. GORTANI – G. LEVI DELLA VIDA – G. MONTALENTI – A. NICEFORO – E. PERSICO – M. PONZO – F. SEVERI – S. TONZIG

"SCIENTIA" is the only review of its type which · has a world wide circulation · deals with the most recent and fundamental problems of all branches of science · can boast among its contributors of the most illustrious men of science in the whole world · publishes the articles in the native language of their Authors (English, French, Italian, German, Spanish). Each issue includes a Supplement *containing the complete French translation of the articles which in the text are published in language other than French.*

"SCIENTIA" has therefore a very strong appeal to the scientific-minded reader all over the world.

Full details and a free back copy will be sent by applying to

"SCIENTIA",—ASSO (Como, Italy)

sending $0.25, or equivalent amount in other currency, preferably in air-mail postage stamps of your country, merely to cover packing and postage. For a number of the current year, please sent $1.20, or equivalent amount in other currency, which will be deducted from the subscription price.

ANNUAL SUBSCRIPTION: $12.80 U.S.A.

OXFORD books of exceptional interest

John Locke and the Way of Ideas

By **JOHN W. YOLTON.** The epistemological and metaphysical doctrines of Locke's *Essay Concerning Human Understanding* are examined against the background of seventeenth and eighteenth century religious and moral pamphleteering. Extensive use is made of manuscript materials from the Lovelace Collection. (*Oxford Classical and Philosophical Monographs*) $4.00

Form in Civilization

Collected Papers on Art and Labor

By **W. R. LETHABY;** *with a foreword by* Lewis Mumford. A classic collection of timeless essays on architecture and the machine age. Lethaby was a contemporary of Geddes, Mackintosh, Ashbee, Voysey, Raymond Unwin, and Barry Parker, men associated in the Arts and Crafts movement which started with Ruskin. $2.75

The Shorter Oxford English Dictionary

Third Edition Revised with Addenda

A superb one-volume abridgement of the 13-volume *Oxford English Dictionary*. Based on historical principles. Etymologies, illustrative quotations, meanings and usage from King Alfred to the present. "There can be no substitute for it. It is the best English dictionary of its size."—MARK VAN DOREN. $25.00

Thumb indexed $26.50

A History of Technology

Volume II: The Mediterranean Civilization and the Middle Ages c. 700 B. C. to A. D. 1500

Edited by **CHARLES SINGER** *and others.* Volume II of a five-volume study. Articles include: Food and Drink, Spinning and Weaving, Roads and Land Travel, Metallurgy, Ceramics, Glass, Furniture, Shipbuilding. ". . . brilliant and lucid . . . provides a balanced perspective."—I. BERNARD COHEN, *New York Times Book Review*. 775 illustrations. $26.90

At all bookstores

OXFORD UNIVERSITY PRESS, Inc., 114 Fifth Ave., N. Y. 11